# PHILOSOPHICAL TRANSACTIONS.

## I. *The Croonian Lecture. On the Functions of the Heart and Arteries. By* Thomas Young, *M. D. For. Sec. R. S.*

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THE mechanical motions, which take place in an animal body, are regulated by the same general laws as the motions of inanimate bodies. Thus the force of gravitation acts precisely in the same manner, and in the same degree, on living as on dead matter; the laws of optics are most accurately observed by all the refractive substances belonging to the eye; and there is no case in which it can be proved, that animated bodies are exempted from any of the affections to which inanimate bodies are liable, except when the powers of life are capable of instituting a process, calculated to overcome those affections, by others, which are commensurate to them, and which are of a contrary tendency. For example, animal bodies are incapable of being frozen by a considerable degree of cold, because animals have the power of generating heat; but the

skin of an animal has no power of generating an acid, or an alkali, to neutralise the action of an alkaline or an acid caustic, and therefore its texture is destroyed by the chemical attraction of such an agent, when it comes into contact with it. As far, therefore, as the functions of animal life depend on the locomotions of the solids or fluids, those functions must be capable of being illustrated by the consideration of the mechanical laws of moving bodies; these laws being fully adequate to the explanation of the connexion between the motive powers, which are employed in the system, and the immediate effects, which they are capable of producing, in the solids or fluids of the body: and it is obvious, that the inquiry, in what manner, and in what degree, the circulation of the blood depends on the muscular and elastic powers of the heart and of the arteries, supposing the nature of those powers to be known, must become simply a question belonging to the most refined departments of the theory of hydraulics.

In examining the functions of the heart and arteries, I shall inquire, in the first place, upon the grounds of the hydraulic investigations which I have already submitted to the Royal Society, what would be the nature of the circulation of the blood, if the whole of the veins and arteries were invariable in their dimensions, like tubes of glass or of bone; in the second place, in what manner the pulse would be transmitted from the heart through the arteries, if they were merely elastic tubes; and in the third place, what actions we can with propriety attribute to the muscular coats of the arteries themselves. I shall lastly add some observations on the disturbances of these motions, which may be supposed to occur in different kinds of inflammations and of fevers.

When we consider the blood vessels as tubes of invariable dimensions, we may suppose, in order to determine the velocity of the blood in their different parts, and the resistances opposed to its motion, that this motion is nearly uniform, since the alternations, arising from the pulsation of the heart, do not materially affect the calculation, especially as they are much less sensible in the smaller vessels than in the larger ones, and the principal part of the resistance arises from these small vessels. We are to consider the blood in the arteries as subjected to a certain pressure, by means of which it is forced into the veins, where the tension is much less considerable; and this pressure, originating from the contractions of the heart, and continued by the tension of the arteries, is almost entirely employed in overcoming the friction of the vessels: for the force required to overcome the inertia of the blood is so inconsiderable, that it may, without impropriety, be wholly neglected. We must therefore inquire, what the magnitude of this pressure is, and what degree of resistance we can suppose to arise from the friction of the internal surface of the blood vessels, or from any other causes of retardation. The magnitude of the pressure has been ascertained by HALES's most interesting experiments on a variety of animals, and may thence be estimated with sufficient accuracy for the human body; and for determining the magnitude of the resistance, I shall employ the theorems which I have deduced from my own experiments on very minute tubes, compared with those which had been made by former observers under different circumstances; together with some comparative experiments on the motion of water and of other fluids in the same tubes.

Dr. HALES infers, from his experiments on quadrupeds of different sizes, that the blood in the human arteries is subjected to a pressure, which is measured by a column of the height of seven feet and a half: in the veins, on the contrary, the pressure appears to amount to about six inches only: so that the force which urges the blood from the greater arteries through the minuter vessels into the large veins, may be considered as equivalent to the pressure of a column of seven feet.

In order to calculate the magnitude of the resistance, it is necessary to determine the dimensions of the arterial system, and the velocity of the blood which flows through it. According to the measurements of KEILL and others, we may take $\frac{3}{4}$ of an inch for the usual diameter of the aorta, and suppose each arterial trunk to be divided into two branches, the diameter of each being about $\frac{4}{5}$ of that of the trunk, (or more accurately $1 : 1.26 = 10^{-.100567}$), and the joint areas of the sections about a fourth part greater, (or $1.2586 : 1 = 10^{.099896}$). This division must be continued twenty nine times, so that the diameter of the thirtieth segment may be only the eleven hundredth part of an inch, that is, nearly large enough to admit two globules of the blood to pass at once. The length of the first segment must be assumed about nine inches, that of the last, the twentieth of an inch only; and supposing the lengths of the intermediate segments to be a series of mean proportionals, each of them must be about one sixth part shorter than the preceding, (or $1 : 1.961 = 10^{-.07776}$), the mean length of the whole forty six inches, the capacity to that of the first segment as 72.71 to 1, and consequently the weight of the blood contained in the arterial system about 9.7

pounds. It is probable that this calculation approaches sufficiently near to the truth: for the whole quantity of blood in the body being about 40 pounds, although some have supposed it only 20, others no less than 100, there is reason to believe that half of this quantity is contained in the veins of the general circulation, and that the other half is divided, nearly in equal proportions, between the pulmonary system and the remaining arteries of the body, so that the arteries of the general circulation may contain about 9 or 10 pounds. HALLER allows 50 pounds of circulating fluid, partly serous, and partly red, and supposes $\frac{1}{5}$ of this to be contained in all the arteries taken together: but in a determination which must be in great measure conjectural we cannot expect perfect accuracy: and according to HALLER's own account of the proportions of the sections of the arteries and veins, the large trunks of the veins appear to be little more than twice as capacious as those of the arteries, and the smaller branches much more nearly equal, so that we cannot attribute to the arterial system less than $\frac{1}{3}$ of the whole blood.

It may be supposed that the heart throws out, at each pulsation, that is about seventy five times in a minute, an ounce and a half of blood: hence the mean velocity in the aorta becomes eight inches and a half in a second: and the velocity in each of the succeeding segments must of course be smaller, in proportion as the joint areas of all the corresponding sections are larger than the area of the aorta: for example, in the last order of vessels, of which the diameter is the eleven hundredth of an inch, the velocity will be one ninety third of an inch: and this result agrees sufficiently well with HALES's observation of the velocity in the capillary arteries of a frog,

which was one ninetieth part of an inch only. It is true, that Haller is disposed to question the accuracy of this observation, and to attribute a much greater velocity to the blood flowing through the capillary vessels, but he did not attempt either to measure the velocity, or to determine it by calculation: nor is this the only instance in which Haller has been led to reason erroneously, from a want of mathematical knowledge: he may, however, have observed the particles of blood moving in the axis of a vessel with a velocity much exceeding the mean velocity of its whole contents. If we calculate upon these foundations, from the formula which I have already laid before the Society, it will appear that the resistance which the friction of the arteries would occasion, if water circulated in them instead of blood, with an equal velocity, must amount to a force equivalent to the pressure of a column of fifteen inches and a half: to this we may add about a fourth for the resistance of the capillary veins, and we may estimate the whole friction for water, at twenty inches. The only considerable part of this force is derived from the term $\frac{2.1126lv}{10^7d^{1.5}}$ in the value of $f$: this term increases for each successive segment in the ratio $1 : 1.49425 = 1 : n$, and the sum of the series is to the first term, as $\frac{n^{30}-1}{n-1}$ to 1. It appears also, that a very small portion only of the resistance is created in the larger vessels: thus, as far as the twentieth division, at the distance of an inch and a quarter only from the extreme capillary arteries, the pressure of a column of one twentieth of an inch only is required for overcoming the whole friction, and at the twenty fifth division, where the artery does not much exceed the diameter of a human hair, the height to which the

water would rise, in a tube fixed laterally into the artery, is only two inches less than in the immediate neighbourhood of the heart.

In order to judge of the comparative resistance produced by fluids of different degrees of viscidity, I employed the same tubes, by means of which I had determined the friction of water, in extreme cases, for ascertaining the effect of different substances held in solution in the water: since it is impossible to make direct experiments on the blood in its natural state, on account of its tendency to coagulate: and those substances which have the power of preventing its coagulation, may naturally be supposed to produce a material change in its viscidity. The diameter of one of the tubes, which was cylindrical, was the fortieth part of an inch: the bore of the other was oval, as is usual in the finest tubes made for thermometers: the section, divided by one fourth of the circumference, gave one hundred and seventy seconds for the mean diameter. I caused some milk, and solutions of sugar of different strength, to pass through these tubes: they were all transmitted much more sparingly than water, with an equal pressure, and the difference was more considerable in the smaller than in the larger tube, as might naturally be expected both from the nature of the resistance, and from the result of GERSTNER's experiments on water at different temperatures. In the first tube the resistance to the motion of milk was three times as great as to that of water, a solution of sugar in five times its weight of water produced twice as much resistance as water; in twice its weight, nearly four times as much as water: but in the narrower tube, the weaker solution of sugar exhibited a resistance five times as great as that of

water, which is more than twice as much as appeared in the larger tube. Hence there can be no doubt that the resistance of the internal surface of the arteries to the motion of the blood must be much greater than would be found in the case of water: and supposing it about four times as great, instead of 20 inches, we shall have 80, for the measure of a column of which the pressure is capable of forcing the blood, in its natural course, through the smaller arteries and veins, which agrees very well with HALES's estimate.

This determination of the probable dimensions of the arterial system, and of the resistances occasioned by its different parts, is in some few respects arbitrary, at the same time that it cannot be materially altered without altering either the whole quantity of blood contained in the body, the diameters of the smallest capillary vessels, the mean number of bifurcations, or the magnitude of the resistance, all of which are here assumed nearly as they have been laid down by former observers: the estimation of the length of the successive segments only is made in such a manner, as to reconcile these data with each other, by means of the experiments and calculations relating to the friction of fluids in pipes. The effect of curvature in increasing the resistance has been hitherto neglected; it can be only sensible in the larger vessels; and supposing the flexures of these to be equivalent to the circumferences of two circles, each two inches in diameter, the radius $q$ being 1, we have $r = \frac{.0000045 p\ v^2\ q^{\frac{1}{8}}}{q} = .0000045 \times 720 \times 64 = .207$, or about one fifth of an inch, for the additional resistance arising from this cause in the case of water, or four fifths for blood, which is a very inconsiderable part of the whole.

It might be questioned whether the experiments, which I have made, with tubes $\frac{1}{172}$ of an inch in diameter, are sufficient for determining, with accuracy, the degree in which the resistance would be increased in tubes, of which the diameter is only one sixth part as great; and it may be doubted whether the analogy, derived from these experiments, can be safely employed as a ground for asserting, that so large a portion of the arterial pressure is employed in overcoming the resistance of the very minute arteries. But it must be remembered, that these experiments are at least conclusive with respect to the arteries larger than the tubes employed in them, and even those which are a little smaller; so that the remaining pressure, as observed in experiments, can only be employed in overcoming the resistance of the minuter arteries and veins, and these observations tend therefore immediately to confirm the analogy drawn from the experiments on the motion of water. It might indeed be asserted, that the viscidity of the blood exceeds that of water in a much greater ratio than that which is here assigned; but this is rendered improbable by some experiments of HALES, in which, when the intestines were laid open, on the side opposite to the mesentery, so that many of the smaller arteries were divided, the quantity of warm water which passed through them with an equal pressure, was only about twelve times as great as that of the blood which flows through them in their natural state; and it is probable, that at least three or four times as much of any fluid must have passed through them in their divided, as in their entire state, unless we suppose that the coats of the divided vessels, like many other muscular parts, are capable of being contracted by the contact of water. In

some other experiments, it was found that a moderate degree of pressure was capable of causing water to exude so copiously through the exhalant vessels of the intestines, that it passed through the aorta with a velocity of about two inches in a second, although these vessels do not naturally allow any passage to the blood: on the other hand, it sometimes happened that very little water would pass through such channels as naturally transmitted a much larger quantity of blood: a circumstance which Dr. HALES very judiciously attributes to the oozing of the water into the cellular membrane surrounding the vessels, by means of which they were compressed, and their diameters lessened. On the whole, it is not improbable, that in some cases the resistance, opposed to the motion of the blood, may exceed that of water in a ratio somewhat greater than I have assigned; but this must be in the minutest of the vessels, while in the larger arteries the disproportion must be less: so that, however we may view the subject, it appears to be established, that the only considerable resistance which the blood experiences, occurs in the extreme capillary arteries, of which the diameter scarcely exceeds the hundredth part of an inch.

We cannot suppose that the dimensions of the sanguiferous system agree uniformly, in all its parts, with the measures which I have laid down; but the truth of the inference is not affected by these variations. For example, there may perhaps be some arteries communicating with veins, of which the diameter exceeds the eleven hundredth of an inch; but there are certainly many others which are much more minute; and the blood, or its more liquid parts, passing through these more slowly, it must move more rapidly in the former, so that the

resistance may in all be equal to the pressure, and the mean velocity may still remain such as is determined by the quantity of blood passing through the aorta. There is indeed some uncertainty in the measure of the globules of the blood, which I have made the basis of the dimensions of the minute arteries: and I have reason to think, that instead of $\frac{1}{2000}$ of an inch, their greatest diameter does not exceed $\frac{1}{3000}$, or even $\frac{1}{3600}$: the general results of the investigation are not however affected by this difference: it will only require us to suppose the subdivisions somewhat more numerous, and the branches shorter.

These are the principal circumstances which require to be considered, with respect to the simple transmission of the blood through the arteries into the veins, without regard to the alternate motions of the heart, and to the elastic and muscular powers of the vessels. I shall next examine the nature and velocity of the propagation of the pulse. The successive transmission of the pulsations of the heart, through the length of the arteries, is so analogous to the motion of the waves on the surface of water, or to that of a sound transmitted through the air, that the same calculations will serve for determining the principal affections of all these kinds of motion; and if the water, which is agitated by waves, is supposed to flow at the same time in a continued stream, and the air which conveys a sound to be carried forwards also in the form of a wind, the similitude will be still stronger. The coats of the arteries may perhaps be considered, without much inaccuracy, as perfectly elastic, that is, as producing a force proportional to the degree in which they are extended beyond their natural dimensions; but it is not impossible that there may be some bodies in nature, which differ materially from this general

law, especially where the distension becomes considerable: thus there may be substances which exhibit a force of tension proportional to the excess of the square, or the cube of their length, beyond a certain given quantity. It is safest therefore to reason upon the elasticity of any substance, from experiments made without any great deviation from the circumstances to which the calculation is to be applied.

For this purpose, we may again employ some of the many excellent experiments contained in Hales's hæmastatics. It appears, that when any small alteration was made in the quantity of blood contained in the arteries of an animal, the height of the column, which measured the pressure, was altered nearly in the same proportion, as far as we are capable of estimating the quantity, which was probably contained in the larger vessels of the animal. Hence it follows, that the velocity of the pulse must be nearly the same as that of an impulse transmitted through an elastic fluid, under the pressure of a column of the same height, as that which measures the actual arterial pressure: that is, equal to that which is acquired by a heavy body falling freely through half this height. In man, this velocity becomes about fifteen feet and a half in a second; to which the progressive motion of the blood itself adds about eight inches; and with this velocity, of at least sixteen feet in a second, it may easily happen that the pulse may appear to arrive at the most distant parts of the body without the intervention of any very perceptible interval of time.

The velocity of the transmission of the pulse being known, it is easy to determine the degree in which the arteries are dilated during its passage through them. The mean velocity of the blood in the aorta being eight inches and a half in a

second, its greatest velocity must be about three times as much, since the contraction of the heart is supposed to occupy only about one third part of the interval between two successive pulsations; and if the velocity of the pulse is sixteen feet in a second, that of the blood itself must be about one eighth part as great; so that the column of blood occupying eight inches may occupy only seven; hence the diameter must increase in the ratio of about fifteen to sixteen. The tension will also become one eighth greater, and the force of the heart must be capable of supporting a column of one hundred and one inches. This force would, however, require to be somewhat increased, from the consideration that the force required at the end of any canal during the reflection of a pulsation or wave of any kind, is twice as great as the force exerted during its transmission, and the force employed in the origination of a wave or pulse in a quiescent fluid, is the same as is required for its reflection; on the other hand, a weaker pulsation, proceeding into a narrower channel, becomes more energetic, so that, from this consideration, a force somewhat smaller would be required in the heart: on the whole, however, it appears probable, that the former of these corrections must be the more considerable, and that the force of the heart must be measured by the pressure of a column, rather more than less than one hundred and one inches high: nor would this force by any means require a strong exertion of muscular power; for it only implies a tension of something less than three pounds for each inch of the circumference of the greatest section of the heart; and supposing the mean thickness half an inch, an equal number of the fibres of some other muscles of the body would be capable of exerting a force of

more than two hundred pounds, in the state of the greatest possible action.

The force, here assigned to each pulsation, agrees extremely well with the inference that may be drawn from an experiment of HALES, on the ascent of the blood in a tube connected with an artery of a horse. The whole height of the column being nine feet, the blood rose about three inches higher during each pulsation, which was repeated fifty or sixty times in a minute: now we may suppose the acceleration to have extended a little beyond the first half of the space thus described, so that two inches were described in two fifths of a second; and if there had been no friction, nor any other cause of retardation, there can be no doubt that at least four inches would have been described in the same time; but the same column of nine feet, if it had been actuated by its own weight, would have described thirty one inches in the same time: consequently the force with which the blood was forced through the artery was nearly one eighth of the whole force of tension, as it appears in the former calculation.

The magnitude of the pulse must diminish in the smaller arteries in the subduplicate proportion of the increase of the joint areas, in the same manner as the intensity of sound is shewn to decrease in diverging from a centre, in the subduplicate ratio of the quantity of matter affected by its motion at the same time. For example, in the arteries of the tenth order, of which the diameter is one thirteenth of an inch, its magnitude must be only one third as great as in the aorta, that is, the greatest progressive velocity of the blood must be eight inches and a half in a second only, and the dilatation one fiftieth part only of the diameter. In the vessels of the

twentieth order, the dilatation does not exceed $\frac{1}{160}$ of the diameter, which is itself the 140th part only of an inch: so that it is not surprising, that HALLER should have been unable to discover any dilatation in vessels of these dimensions, even with the assistance of a powerful microscope. If we estimated the magnitude of the pulse in the aorta, from the excess of the temporary above the mean velocity, which would perhaps be justifiable, that magnitude would become still less considerable.

These calculations agree extremely well with each other, and with experiment, as far as they relate to the power of the heart, and the affections of the smaller arteries. But there is reason to think that the velocity of the pulse in the larger vessels is much more considerable than has been here stated; and their dilatation is also less conspicuous, when they are exposed to view, than it would probably be, if it were as great as is inferred from the velocity here assigned. I have demonstrated, in the hydraulic investigations which I lately laid before the Royal Society, that the velocity of an impulse passing through a tube, consisting of perfectly elastic materials, is half as great as that of a body supposed to have fallen from the given point to the base of the modular column of the tube: and that the height of this column is such that the tube would be extended without limit by its pressure; consequently it must be greater than the height of a column equivalent to the pressure by which the tube is burst. Now it has been ascertained by Dr. HALES, that the pressure, required for bursting one of the carotids of a dog, is equal to that of a column of water one hundred and ninety feet high; nor does he remark that the artery was very materially dilated; and deducting from this height the five feet which express the

actual pressure in the arteries of a dog, the remaining one hundred and eighty five feet will give a velocity of at least fifty four feet in a second, for the propagation of the pulse in the dog. It is not however ascertained, that all the membranes, which may have surrounded the artery in this experiment, are called into action in its ordinary pulsation, much less that the force, developed by their tension, varies precisely according to the general law of perfectly elastic bodies: but this mode of calculation is still amply sufficient to make it probable, that the velocity of the pulsations, in the larger arteries, must amount to at least forty feet in a second, although some very considerable deductions must be made, on account of the resistances of various kinds, which cannot be comprehended in the calculation.

The artery must not be supposed to subside, immediately after each pulsation, precisely to its original dimensions, since it must remain somewhat fuller, in order to supply the capillary arteries, and the veins, in the interval between the two successive pulsations: and in this respect it differs from the motions of a wave through a canal, which is open on both sides: but the difference may be understood, by supposing a partial reflection of the pulse to take place at every point where it meets with any resistance, which will leave a general distension of the artery, without any appearance of a retrograde pulsation.

I shall proceed to inquire, in the third place, into the nature and extent of the functions which are to be attributed to the muscular fibres of the coats of the arteries; and I apprehend that it will appear to be demonstrable, that they are much less concerned in the progressive motion of the blood, than

is almost universally believed. The arguments, which may be employed to prove this, are nearly the same that I have already stated, in examining the motion of a fluid, carried along before a moving body in an open canal; but in the case of an elastic tube, the velocity of the transmission of an impulse being rather diminished than increased by an increase of tension, the reasoning is still stronger and simpler; for it may here be safely asserted, that the anterior parts of the dilatation, which must be forced along by any progressive contraction of the tube, can only advance with the velocity appropriate to the tube, and that its capacity must be proportionate to its length and to the area of its section: now the magnitude of its section must be limited by that degree of tension which is sufficient to force back through the contraction what remains of the displaced fluid, and the length by the difference of the velocity appropriate to the tube, and that with which the contraction advances; consequently if the contraction advance with the velocity of a pulsation, as any contractile action of the arteries must be supposed to do, this length necessarily vanishes, and with it the quantity of the fluid protruded; the whole being forced backwards, by the distending force which is exerted by a very small dilated portion, immediately preceding the contraction. It might indeed be imagined, that the contraction follows the pulsation with a velocity somewhat smaller than its own; but this opinion would stand on no other foundation than mere conjecture, and it would follow, that the pulse would always become more and more full, as it became more distant from the heart; of which we have nothing like evidence: nor would a moderate contraction, even if this supposition were granted, produce any material effect. For

example, if the velocity of the contraction were only half as great as that of the pulsation, which is the most favourable proportion, it would be necessary, taking sixteen feet in a second for the velocity of the pulsation, that the section of the arteries should be contracted to about one half, in order to produce, by their progressive contraction only, the actual velocity of the blood in the aorta; one sixteenth of the blood being carried, in this case, before the contraction: but if the contraction were only such, as to reduce the section of the artery to $\frac{9}{10}$, which is probably more than ever actually happens, the velocity produced would be only about $\frac{1}{80}$ as much; and if the contraction were only to $\frac{99}{100}$, which is a sufficient allowance for the smaller arteries, about $\frac{1}{10000}$ only of the actual velocity in the aorta could be produced in this manner, even upon a supposition much more favourable to the muscular action of the arteries than the actual circumstances. A small addition must be made to the force required for producing the retrograde motion, on account of the friction to be overcome, but the general reasoning is not affected by this correction.

The contraction of the artery might also be supposed to remain after each pulsation, so that the vessel should not be again dilated until the next pulsation, or, in other words, a spontaneous dilatation might be supposed to accompany the pulsation, instead of a contraction: but such a dilatation would be useless in promoting the progressive motion of the blood, since a larger quantity of blood, conveyed to the smaller vessels, without an increased tension, would be ineffectual with respect to the resistances which are to be overcome. It is possible indeed that the muscular fibres of those arteries in which the magnitude of the pulse is sensible, like the fibres of

the heart, may be inactive, or nearly so, during their dilatation, and that they may contract after they have been once distended, with a force which is in a certain degree permanent; the greater momentum of the blood, which accompanies the dilatation, enabling it to enter the minute arteries with equal ease, although assisted by a tension somewhat smaller: so that the same mean velocity may be sustained, as if the arteries were simply elastic, and a little smaller in diameter, with a very little less exertion of the heart. But the distribution of the blood could never be materially diversified by any operation of this kind: for if any artery were for a moment distended by such a variation, so as to exceed its natural diameter by one hundredth part only, a pressure would thence arise equivalent to that of a column about two inches high, which would, in spite of all resistances, immediately dissipate the blood with a considerable velocity, and completely prevent any local accumulation, unless the elastic powers of the vessel itself were diminished; and this is, perhaps, the most important, as well as the best established inference from the doctrine that I have advanced.

It appears that a mola has sometimes been found in the uterus, totally destitute of a heart, in which the blood must have circulated in its usual course through the veins and arteries: in this case it cannot be ascertained whether there was any alternate pulsation, or whether the blood was carried on in a uniform current, in the same manner as the sap of a vegetable probably circulates. If there was a pulsation, it may have been maintained by a contraction of the artery, much more considerable, and slower in its progress than usual; and with the assistance of a spontaneous dilatation; the resistance

in the extreme vessels being also probably much smaller than usual: if the motion was continued, it would lead us to imagine that there may be some structure in the placenta capable of assisting in the propulsion of the blood, as there may possibly be some arrangement in the roots of plants by which they are calculated to promote the ascent of the sap. The circulation in the vessels of the more imperfect animals, in which a great artery supplies the place of a heart, is of a very different nature from that of the more perfect animals: the great artery, which performs the office of the heart, is here possessed of a muscular power commensurate to its functions, and seems to propel the blood, though much more slowly than in other cases, by means of a true peristaltic motion. It appears also from the observations of SPALLANZANI, that in many animals a portion of the aorta, next the heart, is capable of exhibiting a continued pulsation, even when perfectly empty and separated from the heart; but this property is limited to a small part of the artery only, which is obviously capable of being essentially useful in propelling the blood when the valves of the aorta are closed. The muscular power of the termination of the vena cava is also capable of assisting the passage of the blood into the auricle. It is not at all improbable that a muscle of involuntary motion, which had been affected throughout the whole period of life by alternate contractions and relaxations, might retain from habit the tendency to such contractions, even without the necessity of supposing, that the habit was originally formed for a purpose to be obtained by the immediate exertion of the muscular power: but in fact the partial pulsation of the vena cava is perfectly well calculated to promote the temporary repletion of the

auricle, while it must retard, for a moment, the column which is approaching, at a time that it could not be received.

There is no difficulty in imagining what services the muscular coats of the arteries may be capable of performing, without attributing to them any immediate concern in supporting the circulation. For since the quantity of blood in the system is on many accounts perpetually varying, there must be some means of accommodating the blood vessels to their contents. This circumstance was very evident in some of HALES's experiments, when after a certain quantity of blood had been taken away, the height of the column, which measured the tension of the vessels, frequently varied in an irregular manner, before it became stationary at a height proportional to the remaining permanent tension. HALLER also relates, that he has frequently seen the arteries completely empty, although in some of his observations there was probably only a want of red globules in the blood which was flowing through them. Such alterations in the capacity of the different parts of the body are almost always to be attributed to the exertion of a muscular power. A partial contraction of the coats of the smaller arteries may also have an immediate effect on the quantity of blood contained in any part, although very little variation could be produced in this manner by a change of the capacity of the larger vessels.

According to this statement of the powers which are concerned in the circulation, it must be obvious that the nature of the pulse, as perceptible to the touch, must depend almost entirely on the action of the heart, since the state of the arteries can produce very little alteration in its qualities. The greater or less tension of the arterial system may indeed render

the artery itself, when at rest, somewhat harder or softer; and, if the longitudinal fibres give way to the tending force, it may become also tortuous: possibly too a very delicate touch may in some cases perceive a difference in the degree of dilatation, although it is seldom practicable to distinguish the artery, in its quiescent state, from the surrounding parts. But the sensation, which is perceived when the artery is compressed, as usual, by the finger, is by no means to be confounded with the dilatation of the artery; for in this case an obstacle is opposed to the motion of the blood, against which it strikes, with the momentum of a considerable column, almost in the same manner as a stream of water strikes on the valve of the hydraulic ram; and in this manner, neglecting the difference of force arising from the different magnitudes of the sections, the pressure felt by the finger becomes nearly equal and similar to that which is originally exerted by the heart: each pulsation passing under the finger, in the same time, as is required for the contraction of the heart, although a very little later; and more or less so, in proportion as the artery is more or less distant; the artery remaining then at rest for a time equal to that in which the heart is at rest. When therefore an artery appears to throb, or to beat more strongly than usual, the circumstance is only to be explained from its greater dilatation, which allows it to receive a greater portion of the action of the heart, in the same manner as an aneurysm exhibits a very strong pulsation, without any increase of energy, either in itself, or in the neighbouring vessels; and on the other hand, when the pulsations of the artery of a paralytic arm become feeble, we cannot hesitate to attribute the change to its permanent con-

traction, since the enlargement and contraction of the blood-vessels of a limb are well known to attend the increase or diminution of its muscular exertions. There is also another way, in which the diminution of the strength of an artery may increase the apparent magnitude of the pulse, that is, by diminishing the velocity with which the pulsation is transmitted: for we have seen that the magnitude of the pulse is in the inverse ratio of the length of the artery distended at once; and this length is proportional to the velocity of the transmission: but it must be observed, that the force of the pulse striking the finger would not be affected by such a change, except that it might be rendered somewhat fuller and softer, although a considerable throbbing might be felt in the part, from the increased distension of the temporary diameter of the artery. How little a muscular force is necessary for the simple transmission of a pulsation, may easily be shown by placing a finger on the vena saphena, and striking it with the other hand at a distant part; a sensation will then be felt precisely like that of a weak arterial pulsation.

The deviations from the natural state of the circulation, which are now to be cursorily investigated, may be either general or partial; and the general deviations may consist either in a change of the motion of the heart, or of the capacity of the capillary arteries. When the motion of the heart is affected, the quantity of blood transmitted by it may either remain the same as in perfect health, or be diminished, or increased. Supposing it to remain the same, the pulse, if more frequent, must be weaker, and if slower, it must be stronger; but this latter combination is scarcely ever observable; and in the former case, the heart must either never be filled, perhaps

on account of too great irritability, or never be emptied, from the weakness of its muscular powers. But the immediate effect of such a change as this, in the functions depending on the circulation, cannot be very material, and it can only be considered as an indication of a derangement in the nervous and muscular system, which is not likely to lead to any disease of the vital functions. When the quantity of the blood transmitted by the heart is smaller than in health, the arteries must be contracted, until their tension becomes only adequate to propel the blood, through the capillary vessels, with a proportionally smaller velocity, and the veins must of course become distended, unless the muscular coats of the arteries can be sufficiently relaxed to afford a diminished tension, which is probably possible in a very limited degree only. In this state the pulse must be small and weak, and the arteries being partly exhausted, there will probably be a paleness and chilliness of the extremities: until the blood, which is accumulated in the veins, has sufficient power to urge the heart to a greater action, and perhaps, from the vigour which it may have acquired during the remission of its exertions, even to a morbid excess of activity. Hence a contrary state may arise, in which the quantity of blood transmitted by the heart is greater than in perfect health; the pulse will then be full and strong, the arteries being distended, so as to be capable of exerting a pressure sufficient to maintain an increased velocity, and to overcome the consequent increase of resistance; a state which perhaps constitutes the hot fit of fever; and which is probably sometimes removed in consequence of a relaxation of the extreme arteries, which suffer the superfluous blood to pass more easily into the veins. Such a relaxation, when carried

to a morbid extent, may also be a principal cause of another general derangement of the circulation, the motion of the blood being accelerated, and the arteries emptied, so that the pulse may be small and weak, while the veins are overcharged, and the heart exhausted by violent and fruitless efforts to restore the equilibrium; and this state appears to resemble, in many respects, the affections observed in typhus. When, on the contrary, the capillary vessels are contracted, the arteries are again distended, although without the excess of heat which must attend their distension from an increased action of the heart, and possibly without fever: an instance of this appears to be exhibited in the shrinking of the skin, which is frequently observable from the effect of cold, and in the first impression produced by a cold bath: nor is it impossible, that such a contraction may exist in the cold fit of an intermittent, although it seems more probable that a debility of the heart is the primary cause of this affection.

Besides these general causes of derangement, which appear to be more or less concerned in different kinds of fever, there are other more partial ones, which seem to have a similar relation to local inflammations. The most obvious of these changes are such as must be produced by partial dilatations or contractions of the capillary vessels; since, as I have endeavoured to demonstrate, any supposed derangement in the actions of the larger vessels must be excluded from the number of causes which can materially affect the circulation. It cannot be denied, that a diminution of the elastic, or even of the muscular force of the small arteries, must be immediately followed by such a distension as will produce a resistance equal to the pressure: the distension will occasion an increase

of redness, and in most cases pain: the heat will also generally be increased, on account of the increased quantity of blood which will be allowed to pass through the part; and since the hydrostatic pressure of the blood acquires greater force, as the artery becomes more distended, it may be so weak as to continue to give way, like a ligament which has been strained, until supported by the surrounding parts. In this state a larger supply of blood will be ready for any purposes which require it, whether an injury is to be repaired, or a new substance formed; and it is not impossible, that this change in the state of the minute vessels may ultimately produce some change in the properties of the blood itself.

The more the capillary arteries are debilitated and distended, the greater will be the mean velocity of the circulation; but whether or no the velocity will be increased in the vessels which are thus distended, must depend on the extent of the affected part; and it may frequently happen that the velocity may be much more diminished on account of the dilatation of the space which the blood is to occupy, than increased by the diminution of the resistance. And on the other hand, the velocity may be often increased, for a similar reason, at the place of a partial contraction. Hence we may easily understand some of the experiments which Dr. WILSON has related in his valuable treatise on fevers: the application of spirit of wine to a part of the membrane of a frog's foot contracted the capillary arteries, and at the same time accelerated the motion of the blood in them, while in other parts, where inflammation was present, and the vessels were distended, the motion of the blood was slower than usual.

Another species of inflammation may probably be occa-

sioned by a partial constriction or obstruction of the capillary arteries, which must indeed be supposed to exist where the blood has become wholly stagnant, as Dr. Wilson in some instances found it. This obstruction must however be extended to almost all the branches, belonging to some small trunk, in which the pressure remains nearly equal to the tension of the large arteries; for in this case it will happen, that the whole pressure will be continued throughout the obstructed branches, without the subtraction of the most considerable part, which is usually expended in overcoming the resistances dependent on the velocity; so that the small branches will be subjected to a pressure, many times greater than that which they are intended to withstand in the natural state of the circulation; whence it may easily happen that they may be morbidly distended; and this distension may constitute an inflammation, attended by redness and pain. Nor is it impossible, that obstructions of this kind may originate in a vitiated state of the blood itself, although it would be difficult to prove the truth of the conjecture; it seems, however, to be favoured by the observation of Haller, that little clots of globules may often be observed in the arteries, when the circulation is languid, and that they disappear when its vigour is restored, especially after venesection. But if a very small number only of capillary arteries be obstructed, other minute branches will still be capable of receiving the blood, which ought to pass through them, without any great distension or increase of pressure: and this exception is sufficient to explain another experiment of Dr. Wilson, in which a small obstruction, caused by puncturing a membrane with a hot needle, failed to excite an inflammation. This species of inflammation is probably

attended by less heat than the former; and where the obstruction is very great, it may perhaps lead immediately to a mortification, which is called by the Germans "a cold burning."

The most usual causes of inflammation appear to be easily reconcileable with these conjectures. Suppose any considerable part of the body to be affected by cold; the capillary vessels will be contracted, and at the same time the temperature of some parts of their contents will be lowered, from both of which causes the resistance will be increased, and the arteries in general will be more or less overcharged: if then any other part of the system be at the same time debilitated or overheated, its arteries will be liable to be morbidly distended, and an inflammation may thus arise, which may continue till the minute vessels are supported and strengthened, by means of an effusion of coagulable lymph. The immediate effect, either of cold or of heat, may also sometimes produce such a degree of debility in any part, as may lay the foundation of a subsequent inflammation: but the first effect of heat in the bloodvessels appears to be the more ready transmission of the blood into the veins, by means of which they become very observably prominent: and cold, which checks the circulation in the cutaneous vessels, probably occasions a livid hue, by retaining the blood stagnant longer than usual in the capillary vessels of all kinds. It may be objected, that an obstruction of the motion of the blood through a great artery ought, upon these principles, to produce an inflammation in some distant part: but in this case, the blood will still find its way very copiously into the parts supplied by the artery, by means of some collateral branches, which will always admit a much larger quantity of blood than usually passes through them, whenever

a very slight excess of force can be found to carry it on, or when the blood which they contain finds a readier passage than usual, by means of their communication with such parts as are now deprived of their natural supply.

It is difficult to determine, whether blushing is more probably effected by a constriction or by a relaxation of the vessels concerned; it must, however, be chiefly an affection of the smaller vessels, since the larger ones do not contain a sufficient quantity of blood to produce so sudden an effect. Perhaps the capillary vessels are dilated, while the arteries, which are a little larger only, are contracted: possibly too an obstruction may exist at the point of junction of the arteries with the veins; and where the blush is preceded by paleness, such an obstruction is probably the principal cause of the whole affection.

With respect to the tendency of inflammation in general to extend itself to the neighbouring parts, it is scarcely possible to form any reasonable conjecture that can lead to its explanation: this circumstance appears to be placed beyond the reach of any mechanical theory, and to belong rather to some mutual communication of the functions of the nervous system, since it is not inflammation only, that is thus propagated, but a variety of other local affections of a specific nature, which are usually complicated with inflammation, although they may perhaps, in some cases, be independent of it. Inflammations, however, are certainly capable of great diversity in their nature, and it is not to be expected, that any mechanical theory can do more than to afford a probable explanation of the most material circumstances, which are common to all the different species.

Besides these general illustrations of the nature of fevers and inflammations, the theory which has been explained may sometimes be of use, in enabling us to understand the operation of the remedies employed for relieving them. Thus it may be shown, that any diminution of the tension of the arterial system must be propagated from the point at which it begins, as from a centre, nearly in the same manner, and with the same velocity, as an increase of tension, or a pulsation of any kind would be propagated. Hence the effect of venesection must be not only more rapidly, but also more powerfully felt in a neighbouring than in a distant part: and although the mean or permanent tension of the vessels of any part must be the same, from whatever vein the blood may have been drawn, provided that they undergo no local alteration, yet the temporary change, produced by opening a vein in their neighbouhood, may have relieved them so effectually from an excess of pressure, as to allow them to recover their natural tone, which they could not have done without such a partial exhaustion of the neighbouring vessels. But since it seems probable, that the minute arteries are more affected by distension than the veins, there is reason in general to expect a more speedy and efficacious relief in inflammations, from opening an artery than a vein: this operation, however, can seldom be performed without material inconvenience; but it is probably for a similar reason, that greater benefit is often experienced from withdrawing a small portion of blood by means of cupping or of leeches, than a much larger quantity by venesection, since both the former modes of bleeding tend to relieve the arteries, as immediately as the veins, from that distension, which appears to constitute the most essential characteristic of

inflammation. In a case of hemorrhage from one of the sinuses of the brain, a very judicious physician lately prescribed the digitalis: if the effect of this medicine tends principally to diminish the action of the heart, as is commonly supposed, it was more likely to be injurious than beneficial, since a venous plethora must be increased by the inactivity of the heart; but if the digitalis diminishes the general tension of the arteries, in a greater proportion than it affects the motion of the heart, it may possibly be advantageous in venous hemorrhages. We have, however, no sufficient authority for believing, that it has any such effect on the arterial system in general.

Although the arguments, which I have advanced, appear to me sufficient to prove, that, in the ordinary state of the circulation, the muscular powers of the arteries have very little effect in propelling the blood, yet I neither expect nor desire that the prevailing opinion should at once be universally abandoned. I wish, however, to protest once more against a hasty rejection of my theory, from a superficial consideration of cases, like that which has been related by Dr. CLARKE; and to observe again, that the objections, which I have adduced, against the operation of the muscular powers of the arteries in the ordinary circulation, not being applicable to these cases, they are by no means weakened by any inferences which can be drawn from them.

### ERRATA.

In the last volume of the Philosophical Transactions, page 183, line 25, for $\frac{a}{1}$, read $\frac{a}{2}$: page 184, at the end, add, is denoted by *av*: page 186, line 4, for when *ced*, read whence *d*.